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EDITED BY

W. J. GREENSTREET, M.A.

WITH THE CO-OPERATION OF

F. S. MACAULAY, M.A., D.Sc.

AND

PROF. E. T. WHITTAKER, M.A., F.R.S.

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THE SEQUENCE OF THEOREMS IN SCHOOL GEOMETRY.*

BY PROF. T. P. NUNN, D.Sc.

THE subject of my address has recently been "ventilated" in more than one organ of educational opinion; and it is stated that a committee will shortly be assembled to explore the possibility of an escape from the present chaos to the sweet simplicity of an agreed and authoritative sequence.

I know nothing about the constitution of the committee nor about the proposals that are likely to be brought before it; nor had it been announced or foreshadowed when I suggested the subject for discussion to-night. My intention in suggesting that subject was merely to revive and develop further certain proposals brought forward in an address given to the Association† at a time when many of its members were engaged in a vastly more serious discussion elsewhere. I venture, however, to hope that the circumstances I have referred to may add materially to the usefulness of our debate.

I assume as common ground that the school course in geometry should show two main divisions: (1) a heuristic stage in which the chief purpose is to order and clarify the spatial experiences which the pupil has gained from his everyday intercourse with the physical world, to explore the more salient and interesting properties of figures, and to illustrate the useful applications of geometry, as in surveying and "Mongean" geometry; (2) a stage in which the chief purpose is to organise into some kind of logical system the knowledge gained in the earlier stage and to develop it further. In the first stage obvious truths (such as the transversal properties of parallel lines) are freely taken for granted, and deduction is employed mainly to derive from them important and striking truths (such as the constancy of the angle-sum of a triangle) which are not forced upon us by observation. The second stage is marked by an attempt, more or less thorough-going and "rigorous," to explore the connexions between geometrical truths and to exhibit them as the logical consequences of a few simple principles.

About the first stage I shall say nothing except to urge (i) that its range should be liberal, including the simpler truths of tri-dimensional geometry and the properties of figures, such as the conic sections, which were excluded from the Euclidean canon, and (ii) that it should occupy the pupil until he is mature enough really to profit by the second stage—which I interpret as

* The substance of a lecture to the Bristol Branch of the Mathematical Association, 17th March, 1922. Some replies on points raised in the discussion have been incorporated.

† At the Annual Meeting of January, 1917.

meaning that he is not more than two years from the General Schools (or Matriculation) examination. It is upon the second stage that I wish to concentrate attention.

The central purpose of this stage is, we have said, to develop the logic of geometry. A simple illustration may make that purpose clear. Let $ABCD$ be a quadrilateral in which the angles at A, B, C, are right angles by construction; then it may be proved that D is also a right angle. What does the word "prove" mean here? It does not mean that argument is needed to make a boy believe the statement, for there is hardly anything more obviously true. What it means is that the rectangularity of D can be shown not to be an isolated fact but a logical consequence of truths (the fundamental congruence-theorem and the parallel postulate) which he has already accepted. In other words, the argument is not for conviction, but to bring out the logical structure of this particular region of geometrical truth.* Indeed, the example illustrates not inaptly Russell and Whitehead's dictum that we have often more reason to believe our axioms true because true consequences flow from them than to believe in the consequences because they flow from the axioms.

Now, if a boy is to gain profit from logical geometry he must in the first place have a degree of mental maturity which is rarely reached before adolescence, and in the second place its purpose must be carefully explained to him. Good teachers, no doubt, always do explain it, though the requirement is ignored in most of the text-books. But if the explanation is to be really satisfactory it must, I submit, be more philosophical than is usually the case. Pray do not jib at the word philosophical. I mean nothing worse than this: that we should take a good deal of pains to make our pupils realise clearly the logical architecture of the geometrical system. Treated in a sufficiently broad and concrete way, the subject is a fascinating one, appealing strongly to a boy's curiosity and imagination. If he does not get a reasonable amount of intellectual satisfaction out of it, we have a clear indication that he ought not to be doing logical geometry at all.

What is the "logical architecture" of the ordinary geometrical system? It has three main features. The first consists of certain fundamental properties of points, lines and planes—for instance, the fact that a plane is determined by three non-collinear points and that two planes intersect in a straight line. These are the "foundations of geometry," and, as you know, have been the object of an immense amount of patient and subtle scrutiny in recent years. The second feature comprises the axioms and theorems about congruence, especially the congruence of triangles. The third is, in the Euclidean system, some form of the parallel postulate—now-a-days usually the axiom (improperly) called Playfair's. About the last feature I shall shortly make a proposal which is the *fons et origo* of this discourse. But let us proceed towards it in an orderly way.

The study of the foundations of geometry presupposes a logical faculty far more developed than it can generally be in the boy of fourteen or fifteen. It should be taken up, if at all, after matriculation, and in that place I shall briefly consider it. At the beginning of the logical stage it must suffice to call attention to the obvious properties of lines and planes and to point out that they are to be assumed in what follows. The usual theorems about the angles between intersecting lines form a natural appendix to the discussion.

In Euclid's *Elements* the theorems about the congruence of triangles occur at intervals in the first book. Modern text-books rightly bring them together and by that means emphasise their importance and their significance for the

* I do not, of course, deny that the logical coherence of a geometrical system fortifies our belief in the truth of all its parts; my point is merely that this result of a logical inquiry into geometry is not the main reason why we undertake it. To avoid another possible source of misunderstanding, I add that I deliberately ignore here, as too abstract for the school-boy, the standpoint of the truly "pure" geometer. The geometry I have in view is the scientific study of actual space.

geometrical argument. But it is still usual to follow Euclid in proving them by the method of superposition, notwithstanding the severity with which modern geometers have criticised it. Mr. Bertrand Russell, who asserts that it "strikes every intelligent child as a juggle," possibly overestimates the intelligence of children, but does not exaggerate its defects as a principle of proof. The following argument indicates the substance of his objections.

Let two houses be built of the same materials and from the same plans, one (say) in Bristol, the other in Melbourne, and suppose the genius of Aladdin's lamp, in sportive mood, to remove the Bristol house one night and to replace it by the one from Melbourne. Then, though the occupants would no doubt receive a severe shock next morning, passers by would be quite unaware that anything had happened. The argument from superposition says we may conclude that because the Melbourne house, when it reaches Bristol, exactly fills the place of the Bristol house, it filled a space of exactly the same size and shape before it left Melbourne. However true the conclusion may be, it certainly does not follow from the premiss. The fact that the Melbourne house replaces perfectly the one at Bristol proves at most that the same plans carried out twice over *on the same spot*, produce results geometrically indistinguishable; it cannot prove that the Melbourne house, while still in Australia, was "congruent" with its fellow at Bristol. Our belief in that congruence has, surely, an earlier and deeper foundation. Suppose that you were in Melbourne and in the confidence of the humorous demon. Then, if you knew that the two houses had been built from the same plans, you would be certain, *before the event*, that the Melbourne house would exactly replace the one at Bristol. And, if it proved not to do so, you would doubt, not the basis of your conviction, but the workmanship of the builders.

Considerations of this kind suggest that in the interests of clear thinking we should give up the argument from superposition, and place the logical treatment of congruence on its real foundations. Those, I submit, are: (1) the (assumed) possibility that a figure, occurring anywhere, may be exactly repeated anywhere else, and (2) the (assumed) fact that certain elementary constructions, such as drawing a line through two given points, measuring off a given length along a ray, or setting off an angle of given magnitude, can be carried out in only one way. Fusing these assumptions into one, we have the Principle of Congruence: namely, that figures produced by combining the aforesaid elementary constructions in any given (unambiguous) manner are all geometrically equivalent—or, in simpler language, that if a given geometrical construction can be carried out in only one way it produces equivalent figures whether carried out *here* or *there*. For instance, on one side of a given line AB, of length c, it is clearly possible to construct only one triangle ABC, such that AC is of given length b, and the angle between AB and AC of a given magnitude A. It follows from the principle of congruence that triangles drawn to this specification must be equivalent or congruent wherever they may be.

D. Hilbert, in his well-known book,* follows this principle, but limits it to the following axiom: If in two triangles $A'B'=AB$, $A'C'=AC$ and $\angle A'=\angle A$, then $\angle B'=\angle B$ and $\angle C'=\angle C$. From this assumption the equality of BC and $B'C'$ follows and (eventually) all the other congruence-theorems which do not involve the parallel postulate.† I venture to think Hilbert's limitation too drastic for school use. A boy will gain what I have called a more philosophical view of geometry if he is taught to apply the principle in the broader form. For example, it should be used to prove not only the whole of Euclid I. 4, but also the first case of Euclid I. 26, though it

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* *The Foundations of Geometry*. A translation is published by the Open Court Publishing Company.

† He might equally well have started with the assumption that if $AB=A'B'$, $\angle A=\angle A'$, and $\angle B=\angle B'$, then $AC=A'C'$ and $BC=B'C'$. From this it follows that $\angle C=\angle C'$, and Euclid I. 4 can also be deduced.

is a valuable exercise to show subsequently that *either* of these theorems can be deduced from the other. For convenience of reference let the former be called Con. I., the latter Con. II. Then Con. III. (Euclid, I. 5) can at once be deduced from Con. I., and Con. V. (Euclid, I. 8) from Con. I. and III. together. For symmetry's sake I leave a space for Con. IV. (Euclid, I. 6). Personally, I like to deduce this from Con. II. as Con. III. is deduced from Con. I.; the proof used being in each case suggested by Hilbert's proof of Euclid, I. 5. With these five theorems in our hands, we have all the tools needed to develop geometry as far as the principle of congruence alone can support it.

It is, I submit, of great importance to bring out clearly that though the principle of congruence accounts for many striking properties of figures, it does not account for all. For instance, it does not explain the properties of parallelograms, nor Pythagoras's theorem, nor why the angles of a triangle always add up to 180°, nor even the perfectly obvious fact that all the angles of a square must be right angles. For more than two thousand years mathematicians sought to bring these properties within its purview, but were always baffled. Finally, at the beginning of the nineteenth century, it became clear that the problem is insoluble, and that the intractable truths to which I have referred depend upon a second great property of space, additional to and in that sense independent of the property which makes congruent figures possible.

Now what I specially wish to discuss to-night is the question how this second great property should be formulated. Euclid, of course, expressed it in his parallel postulate ("Postulate 5" or "Axiom 12"), and so started a tradition which has been almost universally followed. To challenge a policy laid down by so great a man and hallowed by centuries of acceptance is, I admit, an audacious act. I venture, however, humbly to suggest that Euclid might have served the world better if he had followed another line, and I am about to urge that we should take that line now. I shall argue the question from the standpoint of one whose main interest is in the *teaching* of geometry, but I believe there is a great deal to be said for the proposal from the purely scientific point of view.

I will begin by stressing again the importance of making boys understand clearly the architecture of geometry. From the teaching standpoint it is a serious weakness of Euclid's procedure that he introduces the parallel postulate only to prove the *converse* of a theorem, and thereafter never (I think) mentions it again. It is proved (I. 27) that if a transversal cuts two lines at the same angle the lines cannot meet; but, to get on, we must also know that, if they do not meet, any transversal cuts them both at the same angle. It is to guarantee this conclusion (I. 29) that Axiom 12 (or Playfair's equivalent) makes its solitary appearance. It needs little psychology to see that this procedure does not give the axiom a fair chance. Everyone knows how slow boys are to be convinced that, although a primary proposition has been proved, the truth of its converse remains an open question. It follows that the entrance of the new axiom into the geometrical scheme is psychologically inconspicuous, and that the momentous consequences of admitting it are not clearly realised. And, as I have said, the case is made worse by the fact that, having once admitted it, we never have occasion to notice its presence explicitly again.

Now, from the list of truths not deducible from the principle of congruence, we have omitted by far the most important instance: namely, the existence and properties of similar figures. That figures of very different sizes may yet have exactly the same shape is a fact borne in upon a child from his earliest hours. His mother, as she approaches his cradle, is presented (to use the psychological term) as a series of such figures; it needs no argument or persuasion to make him recognise the cat and the dog in his picture-book; and at a

later stage he accepts maps and plans as the most natural things in the world.* In short, the main facts about similar figures are so familiar, so interesting and so useful in their applications that most good teachers now give them a conspicuous place in the heuristic stage of geometry.† I cannot see how it can be denied that they are most clearly entitled to it and that it is a serious error to ignore them. If teachers were wholly free to obey their teaching-sense, they would probably give similarity a still more important position in their schemes of work. What chiefly deters them is the unhappy tradition which postponed the *logical* treatment of the subject until so late a point that the majority of boys never reached it. Though the Euclidean proofs are gone, the tradition still operates.‡ Its evil effects will not wholly disappear unless we give to the principle of similarity a place in logical geometry corresponding to its psychological importance and its value as an instrument of investigation. That is what I wish to do. Instead of deducing the existence of similar figures from the (assumed) existence of parallels, I propose that we shall deduce the existence and properties of parallels from the (assumed) existence of similar figures. I make the proposal for the reasons explained in this paragraph; and I shall proceed to defend it by arguing (i) that the change would avoid the weakness pointed out in the preceding paragraph, *i.e.* would make the architecture of elementary geometry much clearer, and (ii) that the proofs needed are at least as easy as the proofs they replace, and are sometimes much simpler.

With regard to the first point. The best way to show that one property of space is independent of another is to invent a kind of space in which the latter exists but the former is absent. That is, in effect, what Lobatchevsky did to prove the independence of the parallel postulate. His argument is far too difficult for ordinary school-boys,§ but it is easy to show that congruence may exist where similarity is absent. Take a sphere of such a size that a degree along its equator measures one inch, and draw on it a triangle composed of great circular arcs measuring respectively 6, 8 and 10 inches. (Great circular arcs are not, of course, straight lines, but they correspond to them inasmuch as they mark out the shortest path between two points on a sphere.) If a triangle with sides of these lengths were drawn on a flat sheet of paper, the angles would be 36° 52′, 53° 8′ and 90°, of which the sum is exactly 180°. On the globe the corresponding angles are 36° 54′, 53° 10′ and 90° 2′, making a total of 180° 6′. Thus the triangle on the sphere is slightly different in form from the triangle on the flat; but, like the latter, it would have exactly the same shape wherever drawn. That is to say, the surface of a sphere resembles a plane in admitting endless repetition of the same figure.

The important difference comes into view when we attempt to reproduce the triangle on a different scale—making the sides, for instance, four times as long. On the flat sheet this can be done without disturbing the shape of the triangle, but on the sphere it is otherwise. For if the sides were lengthened to 24, 32 and 40 inches, the angles would be increased to 39° 13′, 55° 28′ and

* The difficulty with regard to maps is, indeed, to persuade him that they are not merely reduced diagrams of the areas they represent.

† The work should include the enlargement and reduction of drawings and a simple treatment of perspective and should incidentally teach the correct technical use of the term "similar." Mr. Fawdry pointed out in the discussion that boys will call all ellipses (for instance) "similar." This, I suggest, is because, according to the ordinary meaning of the word, they *are* similar—just as all triangles are. A new technical term, without misleading associations, would be acceptable. Would *homomorphic* or *identiform* be too alarming?

‡ Some of the Universities now permit candidates for matriculation to refer proofs to the principle of similarity. This is a great advance. It tends to give similarity the place here claimed for it, and it enables teachers to substitute for Euclid's cumbrous proofs of I. 47, III. 35, 36, the simple arguments recommended long ago in Mr. W. C. Fletcher's text-books—from which many of us, in our early days, learnt a great deal. One must also refer gratefully to the help given to the cause by authorities in the service of the Board of Education whose position compels them to be anonymous.

§ I concur with Mr. Carson's opinion (*Mathematical Education*, p. 104) that everyone who proceeds to a University "should gain some slight idea of the nature of non-Euclidean geometry," and I submit that what follows here is not a bad introduction to the subject.

92° 19′, making a total of 187°. Thus the surface of a sphere, being uniform, allows of congruent figures yet does not possess the property which makes similar figures possible. It is clear, therefore, that there is no necessary connexion between the properties of congruence and of similarity. Space as we know it appears to possess both, but it might conceivably have had the first without also having the second.

In conformity with this conclusion it is, then, proposed to organise the whole of geometry on the basis of two (assumed) properties of space. Expressed in popular language they are:

(1) A given figure can be exactly reproduced anywhere.

(2) A given figure can be reproduced anywhere on any (enlarged or diminished) scale.

No further property of space need be assumed, for no property of figures has ever been discovered which cannot be derived from one or both of these.

It cannot, I submit, be denied that logical geometry, built up in the way proposed, would gain greatly in clearness and symmetry. There is no visible kinship between the postulate of congruence and the parallel postulate; but the postulate of similarity resembles and supplements the postulate of congruence in a way that is both obvious and gratifying to the aesthetic sense.

We turn now to the proofs, which are to be based on the axiom that, given a rectilinear figure and any straight line, it is always possible to construct on the given straight line a figure similar to the given figure. Armed with this axiom we can easily show that there are three sets of conditions for similarity between triangles, corresponding, one by one, to the conditions for congruence. The proofs all follow the same lines, so that it will suffice to give the first.

Let $A'B'$ and $A'C'$ have the same ratio to AB and AC respectively, and let $\angle A' = \angle A$. Then we are to prove that $\angle B' = \angle B$, $\angle C' = \angle C$, and $B'C' : BC = A'B' : AB$. Take any line $A''B'' = A'B'$. By the axiom there can be drawn on it a triangle $A''B''C''$ similar to ABC. In that triangle $\angle A'' = \angle A = \angle A'$, and $A''C'' : AC = A''B'' : AB = A'B' : AB = A'C' : AC$; whence $A''C'' = A'C'$. It follows that the triangles $A''B''C''$ and $A'B'C'$ are congruent; so that $\angle B' = \angle B'' = \angle B$, $\angle C' = \angle C'' = \angle C$, and

$$B'C' : BC = B''C'' : BC = A''B'' : AB = A'B' : AB.$$

You will observe that I have said nothing about the commensurability of the magnitudes. As a matter of fact, I think Legendre was right in maintaining that the measurement of ratios is a question for arithmetic, and that we are not necessarily called upon to discuss it in geometry. If we give the proofs of similarity now current in text-books we *must* say something about it; for those proofs deliberately make the false assumption that all magnitudes of the same kind are commensurable. But I count it one of the merits of the proof given above that it does not require us to deal with the question at all. Even if a "rigorous" argument is insisted on, it is easy to meet the requirement by prefacing the above proof with a few axioms embodying the properties of ratios without any reference to their measurement. Nevertheless something must be said somewhere about the measurement of ratios, and it may be conveniently said here and, perhaps, take the following form. If two quantities of the same kind, P and Q, contain respectively p and q units exactly, the ratio of their magnitudes is measured by the fraction p/q. But it is a rare thing to find a quantity which contains the unit an exact number of times, however small the unit may be. What we actually find is, as a rule, that P's magnitude is between p and $p+1$ units, Q's between q and $q+1$. In this case (which, I repeat, is the usual one) we cannot measure the ratio exactly; we can only say that it is between $(p+1)/q$ and $p/(q+1)$. If R and S are two other quantities, we can similarly determine that their ratio lies between $(r+1)/s$ and $r/(s+1)$. Now if there are any fractions which lie between the members both of the first pair and of the second, it is clearly possible that the

two ratios *may* be the same; * and we shall conclude that they *are* the same if such fractions can always be found however small the unit is taken. This is, in fact, the only definition of equal ratios which can be practically applied. Having explained it, you may, if you please, go on to show that there are quantities whose magnitudes cannot, even conceivably, be both measured exactly in terms of the same unit; but the discussion would be a luxury which might well be postponed.

When the fundamental congruence theorems have been proved there is much to be said for going straight on to the corresponding similarity theorems, so that the whole substructure may be laid down before we proceed to build on it. On the other hand, it is important to separate clearly the properties which can be deduced by the congruence principle alone from those which are deducible only with the aid of the principle of similarity. Thus there is also something to be said for postponing the study of similarity for a while. But this is purely a question of expediency, which might affect the numbering of theorems (if it should be decided to give them official numbers), but could not affect their logical order.

The standard theorems deducible from the congruence theorems alone fall naturally into two groups. I should enunciate those of the first group in the following order. (i) If the rays AP and BQ, on the same side of AB, make the angles PAB and QBA together equal to two right angles, then the rays do not meet. (ii) The same may be proved if the angles are together greater than two right angles. (iii) If the lines do meet the angles are together less than two right angles. (*N.B.*—The converse cannot be proved.) (iv) The exterior angle of a triangle is greater than either of the interior opposite angles.

I choose this order because, as Hilbert suggests, (i) can be proved by direct application of the first congruence theorem and because the remaining proofs then become simpler than Euclid's. Moreover, the order seems "prettier" than his. It will, however, be shown below that the whole group can be deduced still more easily from the second similarity theorem, so that the only ground for taking it at this stage is to show how far the authority of the principle of congruence extends.

The second of the two groups consists of Euclid, I. 18-20 and 24. These all depend on (iv) above and therefore could be postponed until similarity has been treated.

It is much more important for my purpose to formulate the standard theorems immediately derived from the fundamental similarity theorems; for, as I have already said, they are to include the doctrine of parallels. I suggest the following order. (i) If any transversal cuts two lines at the same angle those lines do not meet. (This is, of course, equivalent to (i) above.) (ii) On the same supposition, any transversal that intersects the former one also cuts the lines at the same angle. (iii) The angle-sum of a triangle is two right angles. (iv) A triangle can be constructed with angles equal to any three whose sum is two right angles. (v) If two lines in a plane do not meet, any transversal must cut them both at the same angle. (vi) Through a given point there can be drawn only one line which will not meet a given line in the same plane with it. (Playfair's Axiom.)

I will give the proofs of (i) and (v) (Euclid, I. 27, 29). You may find it amusing to work out the others for yourselves.

(i) Let the transversal ABC cut the lines L and M in B and C, and suppose L and M to meet in N. Then the triangles ABN, ACN have two equal angles and are therefore similar. Hence $\angle ANB = \angle ANC$, which is impossible; so the lines cannot meet.

(v) Let $\angle ABL$ be greater than $\angle ACM$; then the interior angles CBL and BCM are together less than two right angles. Let the defect from two right

* This would, for example, be the case if the first pair were 3·463 ... and 3·482 ... while the second pair were 3·471 ... and 3·502 ..., but not if the second pair were 3·483 ... and 3·496 ....

angles be $\angle R$. Then by (iv) a triangle can be constructed whose angles are respectively equal to $\angle B$, $\angle C$ and $\angle R$. Moreover, by the postulate of similarity, a triangle similar to this one can be constructed on BC. But this means that BL and CM must meet, which is contrary to the hypothesis. Hence $\angle ABL$ cannot be greater than $\angle ACM$, etc.

The substitution of a postulate of similarity for the parallel postulate is, as I have admitted, an audacious step. I am anxious, therefore, to plead for it the authority of great names. The first is John Wallis who, in seeking to "demonstrate the fifth postulate of Euclid," followed a method very different from mine but based upon the same assumption.* A much greater authority, Laplace, definitely held that a postulate of similarity is more natural than Euclid's parallel postulate, and considered this view to be confirmed by the remarkable fact that Nature appears to take no account of absolute size, but applies her mechanical laws indifferently to systems almost infinitely big and little. This argument has gained still more force since Laplace's day. Lastly, since this paper was read, Prof. Coulichère of Petrograd has kindly pointed out to me that W. K. Clifford, in his fragmentary but brilliant *Common Sense of the Exact Sciences*, has adumbrated a treatment of geometry similar in principle to the one here advocated. I find (I cannot candidly say with entire pleasure !) that he anticipated the proof I have just given of Euclid, I. 29.

At the end of so long an address only a brief reference can be made to the "foundations of geometry." My view about this part of the subject is that it should be dealt with after matriculation. At that stage it is possible to treat it in accordance with the modern spirit—which has progressed, though many seem not to know it, far beyond Euclid. In short, I recommend a course of the utmost "rigour" based on the work of Pasch, Hilbert, Peano, Veblen and our own Russell and Whitehead. No boy who finds the work unattractive should be compelled to take it, but my experience is that to lads of 17-19 it is often extraordinarily stimulating and interesting. T. P. Nunn.

GLEANINGS FAR AND NEAR.

121. Licensing of the Press. One of these gentlemen (who have never printed their names but to their licenses), said to a geometrician: "I cannot permit the publication of your book: you dare to say, that, between two given points, the shortest line is the straight line. Do you think me such an idiot as not to perceive your allusion? If your work appeared, I should make enemies of all those who find, by crooked ways, an easier admittance into court, than by a straight line. Consider their number!" At this moment the censors in Austria appear singularly inept; for, not long ago, they condemned as heretical, two books, one of which, entitled *Principes de la Trigonométrie*, the censor would not allow to be printed, because the *Trinity*, which he imagined to be included in trigonometry, was not permitted to be discussed; and the other, on the *Destruction of Insects*, he insisted had a covert allusion to the Jesuits. . . . Malebranche could not get a license for his *Recherches après la Verité* until Mezeray approved of it as a work on Geometry. . . .—Disraeli, *Curiosities of Literature*, p. 254.

122. Mr. Shirley (alias Dr. Shirley) . . . subsists, as other authors must expect, by a sort of Geometry.—Dunton's *Life and Errors*, i. 185.

* It wil be of interest to quote his actual words: "Praesumo tandem . . . ut communem notionem

Datae unicunque Figurae. Similem aliam cujuscunque magnitudinis poss bilem esse.

Hoc enim (propter quantitates continuas in infinitum divisibiles, pariter atque in infinitum augibiles) videtur ipsa Quantitatis natura fluere; figuram scilicet quamlibet continue posse (retenta figurae specie) tam minui, tam augeri in infinitum." From *Opera* (1693), vol. ii. p. 674. I have followed up the references in Bonola's invaluable *Non-Euclidean Geometry* (Open Court Series).

DIFFERENTIAL EQUATIONS IN MECHANICS AND IN PHYSICS.

A Presidential Address, delivered before the London Branch of the Mathematical Association, by

PROFESSOR A. R. FORSYTH, F.R.S.

IN accepting the honour of being your President, I was told that the office carried only a single duty—that of delivering an address at a then distant date: even the selection of the topic could be deferred for a time. So there lay before me a primrose path of dalliance. I had some hesitation about the selection of a topic. One decision, however, was immediate; my determination was instantly framed to avoid any discussion of the place of mathematics in education, an unending subject, of which perhaps you have heard only too much. It would have been possible to select some special topic, such as functionality, in its historical development; and you would have had a mere mathematical lecture. I have preferred to choose a general subject: my text seems highly technical in phrase, but I shall not keep very closely to it; my purpose rather is to fall back upon my own experience over a wide range of teaching and to exhibit for your consideration some reflexions based on that experience. Moreover, technical as its title is, my address will contain no mathematical symbols. Though my work as a teacher and writer is largely associated with differential equations—probably in the course of a year I receive (with a request for solution) a greater number of insoluble equations than any one here—the reason for its selection is not to be found in any specially personal relation. The reason is that the particular processes affect many subjects in mathematics, and consequently allow general comparisons to be made, as well as particular inferences to be drawn: they constitute a method as powerful in operation and effective in results as any within our domain.

On present practice, we might be tempted to assume that progress in the subjects concerned is impossible without the use of differential equations; for they seem all-pervading. Yet such an illusion would be at once dispelled by a little remembrance of what was achieved by the ancient Greeks, to mention only one of the old mathematical nationalities, innocent of any knowledge of the differential calculus. It may be that, in some distant future, they will be superseded as useful instruments and will have subsided into an object of study solely by historians and antiquarians. That future does not seem at hand. A brief consideration of our active methods of mathematical analysis, in its applications to the phenomena of nature, will show how inevitable differential equations have become in mechanics and in physics.

When we want the complete solution of any problem that needs mathematical treatment, we demand a full knowledge of all the quantities specific to the problem; and these are to be expressed in terms of the variable or variables upon which they depend, the expression in extreme cases being of a form that admits of numerical calculation with a minimum of labour. This full knowledge is to be not merely a record of the past, by which it can be tested as to accuracy, but also it is to be a useful prophetic guide to the future; and the mathematician has only his present in which he can achieve the knowledge. He cannot expect to obtain it by a single effort and in a single stage; the circumstances are nearly always too complicated. So there usually are three stages. One of these consists of the selection of a theory or an appropriate portion of a theory, taken for granted before he begins—the laws of motion, a theory of gravitation, of the aether, of relativity, of the electromagnetic field; and the mathematician, economic of his labour, no more spends time in the detailed examination of his theory though it is the very foundation of

his work, than does a sailor in testing the stability of his ship before each voyage. The next stage is occupied with general calculations, which have certain characteristics in common whatever the problem may be: and it is in this stage that the professional mathematician finds the most congenial scope for the exercise of his powers. The last stage—and, though he does not always know it to be so, the most important stage for the problem if the work is not to remain a mere piece of calculation—is occupied in applying his general analysis to his particular problem by utilising all relevant facts and data so as to make the result precise: and he may occasionally find that alleged facts and data are not always consistent with one another, in which case there is trouble in his intellectual household. Let me deal with these three stages in turn.

I.

First, then, as to the theory which is selected, often by the exercise of no more than habit. It is at once necessary to indicate more precisely what I mean by a theory, and what is its significance in the perspective of the whole investigation. The word itself is used with varied meanings in mathematics and physics. Thus we have a theory of numbers, a theory of partitions, a theory of equations; in such cases, the word implies an ordered collection of results and rules belonging to particular domains of knowledge. There is a theory of combination of observations, which ultimately amounts to a practice of utilising inconsistent facts; and, however much disguised under long and wordy controversies in the past, the usual practice is based upon one initial arbitrary assumption—that of the arithmetic mean—though there are other assumptions, equally good from a mathematical point of view, though less suited to merely numerical simplicity. Or, again, we can speak of Riemann's theory of functions, meaning thereby an ordered body of doctrine, set out and developed according to a fundamental group of ideas special to the expositor. In none of such cases can there arise the possibility of casting the so-called theory aside. The detailed inferences may be improved and increased by added knowledge; but the theory remains the common and permanent possession of all who are interested in the matter. There can be no question or dispute as to its accuracy or truth.

But there is a very different story to tell when we come to many of the so-called theories that have figured through the ages in mechanics and in physics. The highway of human knowledge is strewn with the wreckage of discarded theories. They serve their purpose for a time, perhaps for a long time; then there may come the tragedy of the theory killed by new facts. If so, a new theory is built up, perhaps on the ruins of the old; the process is often repeated, because what is new to-day may become stale and unprofitable after a few to-morrows. What then is the actual significance of such theories, and why are they superseded by successors that may be expected to fade in their turn? To my mind, the explanation lies in their attribute of being working hypotheses and nothing more than working hypotheses. A theory does not explain a fact; the fact props the theory when the latter provides an inference consistent with the fact. The theory is a good working hypothesis when it leads to inferences prophetic before the fact, such as the Newtonian hypothesis in its relation to the return of the comet now called Halley's. Yet there is a wide gap between the judgment that accepts, and continues to accept, a theory as a working hypothesis, and the judgment that declares a theory as established truth. Again my declaration is individual; and, when once diverging individual declarations are made, the field is set for controversy, so that I may be treading on dangerous ground. Anyhow, my judgment is that a theory can only be accepted, in so far as its predictions accord with facts, previously known, or subsequently discovered.

Further, I hold that a theory cannot be regarded as established until two conditions are satisfied. The first of these conditions is that no other possible

theory can be devised which will equally be in accordance with all the known facts—a condition which renders the establishment of a theory a matter of the utmost difficulty; yet it should be remembered that the solution of the universe by a theory cannot be expected to be attainable without difficulties appalling to the limitations of human knowledge and human powers. The second of the conditions renders almost impossible the establishment of such a theory, even if it is regarded as unique, solely because no other effective theory is known; it is that the theory must be in accord with all the pertinent facts known, as well as unknown, and that all deduced inferences, capable of the most accurate observations possible, must agree with those observations up to the utmost limit of accuracy attainable.

Let me take an example belonging to the cosmology of the universe, in its history, not in its successive phases of speculation, however fascinating these may now be in their antiquarian interest. There are theories of cycles, epicycles, excentrics, the circular orbits of the moon, the sun, the five planets (or wandering stars), connected with a principle of number and time: they served their purpose among the generations of philosophers and astronomers of their day. What has survived, as a permanent contribution to knowledge, is the record of observations, as accurate as opportunity and geometrical inference would then allow; the theories are dead now, though accepted in the hey-day of Greek philosophy. Then came the time of Hipparchus, with a modified theory, and the much later era of Ptolemy, with further improvements in theory and observation as regards the moon in particular and the five planets; and the theory, with the important earth as the centre of the universe, held sway, during the long and dark western middle ages, as the Ptolemaic system. And so thought continued, or was suspended, until the beginning of the sixteenth century; and the old Ptolemaic theory died, because of the new Copernican theory which made the sun the resting centre of the universe, and made all the planets, even the earth, move round that sun. And though the theory was a new doctrine, it was not an established truth; for a time it was an unestablished heresy, as Galileo was to find. It still used epicycles and excentrics: it had nothing of the notion of universal gravitation: whether as heresy or as doctrine, it had, like so many old theories, to make way for newer theories.

The succession of theories recalls the cult of the *Golden Bough* in the old Arician grove near the lake of Nemi, having, as its genius,

> The priest who slew the slayer
> And shall himself be slain.

After this brief and completely inadequate sketch of the development of even only a single branch of mathematical knowledge, let me skip over the centuries, temporarily ignoring Kepler, and Newton, and all the contributors (whether observers, or mathematicians, or passing philosophers) to the theory of gravitation. Not a little thought and not a little ingenuity have been devoted to attempts at explaining gravitation, itself taken as the explanation of the mechanics of the universe; but these attempts at theories are now, many of them in their turn, the flowers of yester-year.

I take a later and conspicuous example, at my peril. In the last few years, the physical section of the scientific world has been profoundly stirred by the theory of general relativity. In what is often regarded as his definite exposition of the principle of general relativity, Prof. Einstein says (I give a free paraphrase of the passage, keeping accurately to one all-important word):

"The facts that when the equations, following mathematically from the postulation of general relativity, are applied to the motion of a single body round the sun, they give Newton's law of attraction as a first approximation and furnish an explanation of the movement of the perihelion of Mercury (discovered by Leverrier) as a second approximation, must in my opinion be convincing evidence of the physical *correctness* of the theory."

Further, since he wrote the memoir which has just been quoted, there has been the experimental verification of the gravitational deviation of a ray of light passing near the limb of the sun, which his theory had predicted, a fact which (though emerging from only a single opportunity) adds strength to his last sentence.

With all respect to the illustrious author, I cannot regard the correctness of the theory to be established by two striking verifications. The inferences from the theory proved to be in accord with one known fact and with one fact to be discovered. To me, the theory thereby becomes a working hypothesis; but, to me, it is not established truth. I heard his lecture at King's College last June and noted its general tone; and I am aware of the eager confidence of some of his disciples, who occasionally appear to me to be more royalist than a Stuart King. Having thus perhaps fallen into trouble, let me make a further confession of my own mathematical heresy and heterodoxy. In spite of the prediction concerning Halley's comet, made fifty-three years before its reappearance, and in spite of the discovery of Neptune by Adams and Leverrier, two scientific events inferred from the Newtonian theory of gravitation, which are certainly not less striking than recent events, I have never regarded the Newtonian theory as more than a good working hypothesis. Even those who have regarded it, or who still regard it, as established truth must now for a time at least be prepared to face partial disestablishment. Another and a newer theory has provided an explanation, which the older theory has not provided, to be in accord with fact. In a tract published in 1831, Sir George Airy declared that the theory of gravitation is certainly "true" and, likewise, the undulatory theory of optics; I sometimes wonder how he would have dealt with the Report on the relativity theory of gravitation, written by his latest successor in the Plumian chair at Cambridge.

Is, then, every theory merely vanity and vexation of mind? It seems to me less disappointing that a cautious intellect should use a good working hypothesis for the increase of knowledge and the slow growth of wisdom than that it should prematurely declare the theory to be established truth and proceed to rule out doubts, questions, alternatives. An over-pontifical spirit might be saved from the trouble of some later disillusion if it would only act upon the ancient maxim *cavendo tutus*. Even the special theory of relativity was transitory.

II.

Coming to the second stage of my remarks, let me assume that we possess a good working hypothesis or theory which, under management, can deal effectively with our supposed enquiry. How are we to proceed? Occasionally there are broad general conclusions which can be applied; such are to be found in mechanics, in the form of the doctrine of conservation of energy, conservation of moment of momentum, or in the theory of light, in the form of Huygens' principle that the time of passage between two points in the actual path of a ray of light through any medium is stationary for all small hypothetical variations of the path. Such items of assistance are, however, not frequent; and we must be ready to do without them, so that we are driven into the mathematical expression of a fitting number of properties characteristic of the problem. There would be a fascination in writing down complete results by means of rules as final and definite as the contents of the multiplication table; but the quantities or magnitudes vary in such a way as to defy rules of this comprehensive type. So, instead of dealing with the elusive quantities, we take account of their changes, and especially of their rates of change in time and space, due regard being paid to appropriate selection of the space for the purpose in hand. But usually the consideration of the rate of change is not adequate; we have to consider the rate of the variation in this very rate of change, corresponding to an acceleration and not merely to a velocity. And mostly it happens that the broad inferences from

the theory or the working hypothesis enable us to formulate mathematically some property or properties of these secondary rates of change.

Very frequently the expressions of the property or properties are formulated by means of quantities connected with impulses or continuous forces, such as velocities or accelerations which, by their very nature, are differential coefficients of varying magnitudes; the results, which emerge from this process, are differential equations.

Consider, for a moment, the movements of a conservative dynamical system. Originally we postulate, in such form as suits reason or preference, general laws of motion that apply to all such systems; we proceed, laboriously, to construct as far as possible a method that shall be generally comprehensive of such systems; and before we descend to particulars, we may be in possession of some such methods as Lagrange's equations of motion. But whether this process is adopted, or whether we work out matters from the beginning without the help of the general method, we cannot avoid the central region, however it is reached; we are face to face with differential equations at some passage or other in practically all but the simplest problems in mechanics and physics. And if we are to progress further, these equations must be solved or integrated; so, therefore, we need such skill or such knowledge as can be provided by the mathematician.

Now it is a piece of good fortune that, in many of our investigations concerning the phenomena of nature, the differential equations which occur are of relatively simple types; the simplest of all are those which are usually called linear, though it must not be assumed that an equation which is of simple type necessarily has a similarly simple solution. But we can proceed to solve them; and intellectual satisfaction is naturally all the greater when the equation is easily solved in the most general fashion. As to this type of soluble equation, I shall have something to say later, almost amounting to a grumble.

Unhappily, there also occur types of equations, not specially complex in form, for the complete and useful integration of which the whole army of mathematicians can provide no effective terrors. Let me instance the comparatively simple telegraph-telephone equation in its general form as used at the present time. Your superbly skilled mathematician will provide a quite general solution in the form of a multiple integral involving a couple of arbitrary functions which he tells you are unreservedly at your disposal. Not merely so; but, aided by the knowledge from some brother mathematician whose interests lie mainly in the rarer region of pure theory, he can declare that the solution he has provided is the most comprehensive that could possibly exist. It sounds splendid, and it looks impressive. But the glow of satisfaction begins to diminish when you examine these gift-functions, so as to utilise them for your own purposes; to make them specifically useful, you would require to obtain an amount of detailed observational knowledge which cannot be obtained and which, had it been possessed, would have rendered the whole mathematical investigation unnecessary. The monumental mathematical solution remains; but how is the poor investigator to help himself? He need not feel stranded; probably at this stage he knows the kind of answer to be expected or desired, quite unlike the magnificent integral which has been offered to him; and there are adequate means of proceeding to something which, though not fully general, will contain sufficient elements of generality to satisfy the demands of his immediate investigation.

And what of the equations with which nothing can be done that is complete? (I may say that these are mostly the kinds of equations that are sent to me by correspondents, who seem to expect a neat pocket-formula in reply, and who are bound to be disappointed.) There remains the method of approximation in its various forms; again, each form provides sufficient elements of generality. The method has one obvious advantage; for the closeness of the approximation in cases where physical measurements can be taken is governed

by the degree of accuracy of the measurements, and the approximation can be terminated accordingly.

Now let me return for a little to equations, which can be integrated completely and the integrals of which are not complicated; and, to keep my meaning clear, let me deal with a particular instance. All of you either are, or at some time have been, acquainted with the simple linear partial differential equation of the second order characteristic of two-dimensional potential in free space. The ordinary mathematical student can, and does, obtain the most general integral of the equation without any difficulty; and not infrequently he becomes acquainted with the two or three fundamental properties of the integral; in addition, he acquires some knowledge of the significance of the first differential coefficient (let me call it the derivative) of the integral function, all merely as a piece of pure mathematics.

Mark what happens in fact. One author, mainly interested in the dynamics of rigid bodies (which incongruously include membranes that can vibrate), writes out the pure mathematics from the beginning and establishes all the properties under a heading entitled "conjugate functions"; and then proceeds to apply the properties to the vibrations of membranes, and also to the theory of inversion in its application to paths of plane motion, finding that the derivative measures the ratio of the velocities in the related paths. Another author, mainly interested in electricity and magnetism, again writes out the pure mathematics in full, regarding the subject, sometimes as "conjugate functions," sometimes as connected with the complex variable; and he proceeds to apply the calculations to special problems in electrostatics, explaining how the derivative gives a measure of the electric intensity and of the surface-density on a conductor. Another author writes out the pure mathematics more sparingly, though the matter is there, all the same; and among other topics, he applies the results to torsion problems in elasticity. A fourth author again writes out the pure mathematics, still sparingly, but (as before) dealing with the essentials; and proceeds to apply the analysis to the steady conduction of heat, finding that the derivative leads to a measure of the flux of heat at any point in a cylindrical body. Yet another author again writes out the pure mathematics, without undue restriction on fulness (perhaps I shall say that several authors do so); and, substituting water for heat, proceeds to deal fully with irrotational fluid motion in two dimensions, finding that the derivative provides a measure of the velocity at any point. That not too many illustrations may prove overwhelming, let me only refer to a sixth typical author, who writes out the pure mathematics with a fulness that obviously is not burdened by reluctance; but he passes to the domain of analysis, and proceeds to apply the results to what is technically called conformal representation, and is, in more homely fashion, called a map, under any projection that is accurate everywhere in the immediate vicinity of any place; and he finds that the first derivative gives the measure of the magnification between the map and the original at any point.

And all these developments are only some of the deductions that arise out of the physical interpretations of a single simple partial differential equation of the second order, taken as a particular example. Other examples of the corresponding practice of independent repetition of pure mathematics in expositions of apparently distinct branches of applied mathematics readily will occur to many minds; need I do more than mention Fourier Series, Zonal Harmonics, Bessel Functions, and other branches of Harmonic Analysis? In each of them, the small body of pure mathematics is the same throughout: it is only in vocabulary that the respective applications differ and appear to have no relation with one another.

In these days of rather particular specialisation, a student working at one subject in considerable detail might not unreasonably be tempted to regard any one of the methods, perhaps every one of the methods, as having been devised expressly for his subject; and the temptation to do so would not be

lessened if they were gathered together as a disjointed collection for that subject. I am inclined to think that his grasp of his subject would be strengthened rather than weakened if he studied the applications of the analysis in many other subjects; and to make this easily possible for an English student, all that is needed is the possession of an English book such as Weber's edition of Riemann's *Partial Differential Equations of Mathematical Physics*. It need not have so much elaborate pure mathematics, included solely for their own sake; it need not pursue the applications of the mathematics up to the boundaries of knowledge; it would be just a useful treatise of natural philosophy, written by one who knows his mathematics, his mechanics, and specially his physics. Will not some mathematical physicist, not too set in the ancient ways of present practice, confer this boon on the coming generations of English students?

III.

Perhaps I have wandered rather far from my main topic; so I return to it, proceeding to the third and final stage of our supposed investigation. Let me assume (sometimes it is a large assumption) that the differential equations have been completely solved; in that event, the resulting analysis will contain a number of undetermined (or arbitrary) constants or else a number of undetermined (or arbitrary) functions, which have arisen during the inverse processes of integration. These processes are, of course, the necessary complement of the differential processes adopted in the initial formulation of the problem; there, we were obliged to deal, not with the quantities defining the ultimate complete knowledge for the problem, but, at best, with the changing rates of change of the quantities; consequently, the inverse return to the magnitudes themselves must be made.

There remains therefore the necessity of determining the arbitrary constants or arbitrary functions in our provisional solution. This determination is effected by means of what often are called the initial conditions in mechanics and the boundary conditions in physics, these being really constituted of the body of data particularising the problem—in other words, it is effected by inserting the particular facts in the general conclusions that have been obtained.

Now the facts, which are provided in different branches of applied mathematics, vary gravely in their tractability. Thus, in the science of statistics, they are bound to be purely arithmetical and terribly discrete; and in an attempt to deal continuously with them, the successive numbers can be subject to all sorts of uncontrollable variations. For instance, the number of persons dwelling in a town from day to day will vary, owing to deaths, to births, to migrations inwards, migrations outwards; in such cases, belonging to a region that ranges from actuarial work to biometrics, the real and great difficulty lies in accommodating the mathematics to the facts. In one department of mechanics, which usually is called physical astronomy, the main provision of facts through observations is superb; and the use of facts might be described as straightforward but terribly laborious. But even then, the provision of facts is not complete; for example, there is not an adequate body of fact to determine purely periodic orbits in even the simplest form of the problem of three bodies as studied initially by Sir G. H. Darwin. Thus, to take one particular question—what is the relation between the constant of energy and the apsidal distance in order that a plane orbit in the restricted problem of the three bodies may be purely periodic? My own conviction (I cannot prove it though, as Fitzgerald in like circumstance said on a famous occasion, "I feel it in my bones")—my own conviction is that the two constants are connected by a transcendental equation or satisfy two simultaneous transcendental equations; all that has been definitely achieved is Darwin's very approximate purely arithmetical determination of a number of isolated

solutions. Or is a real solution possible only for specific values of the ratio of the two main masses ?

To take another instance from modern mechanics, consider the development of the mathematical theory of aviation. The aeroplane is treated as a solid moving in a fluid ; and, *e.g.*, something has to be done by way of formulating a law of the resistance in the slipping movement. What is a law for this purpose ? Scientific method consists in the aggregation of facts, to be found by experiment, in their classification, in the detection of possible relations and sequences, and then leads (by the exercise of intellect, intuition, imagination, inspired guessing, call it what you will) to the formulation of a statement, which temporarily embodies these relations. But the formulated statement may vary from one body to another, may vary with changes in only a single body ; and so it is subject to repeated experiments. The facts are hard to get, though the mathematical treatment may be relatively efficient.

Not infrequently the mathematician is blamed quite summarily for one or other of two reasons—one of them is because of his lack of skill in solving impossible problems—the other of them, because of his superfluity of skill in producing indubitably accurate but useless solutions of possible problems. In any case, he must as a rule take his facts from others ; for it is only in relatively rare cases that now he himself is a competent observer, so extended is the range of observations. As a consequence, he is at the mercy of the facts provided : he is always subject to mistakes in his own work, these however being subject to his control or correction ; but if the data given are not in accord with one another, he becomes their victim for the time being, for he declines to change his facts when they lead to incongruous results, and he cannot make out a reason for the lack of accord between the facts. Let me give you an instance out of my own experience, not otherwise stating names. On one occasion, a considerable number of years back, I received a question from a man, who was a reputable mathematician with wide practical interests. His question was stated, as sometimes is not the case, in sufficiently precise mathematical form. For its answer, the use of the calculus of variations was required, together with the integration of a not very tractable differential equation. At the same time, he sent me a statement of data, representing three distinct facts with which the result should be in accord. My initial expectation was that the problem would prove to be of the customary type, either non-soluble or, at best, possessing only a cumbersome solution. To my surprise, the solution turned out to be not merely possible, but also comparatively simple. But on proceeding to determine the arbitrary constants in the general solution by using the specific facts, I found to my dismay that the results were inconsistent with one another. Much review of my mathematical work, which happened to be accurate, left me with the same conclusion ; and I had to write telling him that, if one of the facts were omitted, he would have one inference, and that if a different fact were omitted, he would have a different inference ; telling him also that, for his question as stated, he could not have all three facts together. After a time there came his reply, both grateful and welcome ; it was to the effect that one of the three alleged facts was wrong, having been an inference derived from misinterpreting the result of an experiment, and must therefore be withdrawn. Intellectual harmony was restored, ending the temporary chaos.

One further consideration I wish to submit to you. Our mathematics can test theories, hypotheses, suppositions, laws, whatever they may be called, can verify them or the reverse, but cannot establish them. The facts give the means of test, sometimes even of critical test between rival theories : need I adduce, in illustration, the corpuscular theory of light, the Fresnel-Young undulatory theory of light : Maxwell's electromagnetic theory of light, with its subsequent modifications and developments through Hertz and through Röntgen's discoveries ? or the still-growing electron theory of matter (or non-matter) with the rather speculative developments of the theoretical

resolution of the elements in physical nature? Correct facts, based upon observations as minutely accurate as is humanly possible with the most precise care (and, to this generation, the minuteness seems a marvel; but it has ever been so, in the progress of acquired knowledge), correct facts may be placed into, or in the way of, a theory; but the extracted result from the facts will not be more than what was inserted. Our mathematics will not create new facts, in mechanics or in physics; it may, with the aid of a blend of half-obscure metaphysics, spread a sort of half-obscurity over old theories. It may, indeed, predict new inferences, apparently of an entirely mathematical character, apparently also beyond what was recognised as being inserted; but if these are to be significant, not solely as mathematical exercises, they must be such as to submit themselves ultimately, in some form or other, to observation and experiment. Such a phenomenon does not often appear in physical science. The most conspicuous example that occurs to my memory is Sir William Hamilton's inference from Fresnel's wave-theory, of conical refraction; both external, as coming out of a biaxal crystal at a conical point on the wave-surface; and internal, as entering the crystal normally to the singular tangent-plane to the wave-surface. There had been no such observation, in fact; there had previously been no such anticipation of such a possibility. But the prophetic inferences made by the mathematician were verified by the now historical observations of the physicist.

In all my criticisms and suggestions of criticism, I have tried to keep within the ample boundary of the title given to the address, and have abstained deliberately from any constructive statement of the aims, the purposes, and the range of pure mathematics, within the domain of which I am often supposed to be restricted. Much could be said upon that range; much could be claimed for it as an experimental science, though its experiments can be conducted without even cork or test-tube, certainly without the most refined apparatus of the physical laboratory or the chemical laboratory. But its quest is not mine to-day. My old training at Cambridge—now seeming a little remote—came at the verge of a transition period, when men were ceasing to think solely (but had not quite ceased to think almost solely) of success in mathematical examinations as a supreme occupation for an intelligence of a mathematical bent. The old training remains in its influence; and, to it, homage must be paid. But a new spirit had begun to stir; thought, rather than facility of calculation, was beginning to break upon that section of the Cambridge world. The cleverness in the solution of tripos problems, and even the mere manipulation of analytical processes, probably have diminished since that day. Not all the developments may have proceeded along the lines that were expected. I fought hard for the modification of the teaching of geometry, which once was all theory and logic, without practice: and sometimes I seem to have observed that it has come to be mostly practice, with little theory or logic except among the extreme experts. Perhaps it is one more of the frequent instances of the former mild radical becoming a later mild conservative. Whatever the external judgment may be, I submit my opinions and estimates, out of my own experience, to your consideration. My passing position of honour does not pledge you to their acceptance, in whole or in detail; but my wish is that your Association may share my belief that the desire for clear comprehension, rather than for rapid facility in artifice, is one of the best of guides to the acquisition and the increase of knowledge.

A. R. Forsyth.

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123. In *N. & Q.*, I. i. p. 143, is an advertisement: Books and Odd Vols., wanted to purchase: . . . Dr. Austin's *Critical Examination of the First Six Books of Euclid* (date not known). Sir T. Heath gives the date as 1781: *An Examination of . . . Euclid's Elements.*

GRAPHS OF TRIGONOMETRICAL EXPRESSIONS.

By F. G. Hall, M.A.

A study of the questions on this subject set in the recent geometry papers of the London Intermediate Examinations will show that there is considerable danger that what should be the chief aim in the subject may be subordinated to the subsidiary aim of affording practice in evaluating and plotting results.

The chief aim should be, I think, a careful study of those important curves in Physics and in Higher Mathematics which best lend themselves to trigonometrical rather than to algebraic treatment. In the actual plotting of these curves much varied practice in the use of tables will, of course, be obtained; but this should be considered of distinctly minor importance.

Considering the questions set, say from 1910 to 1921, it will be seen that for one good question like: "Express $3\sin x - 4\cos x$ in the form $a\sin(x+A)$, and hence sketch its graph from $x=0°$ to $x=360°$," there are many involving the graphs of expressions such as $2\cos x+\cos^2 x$, $\sin x+\sin 3x$, $\frac{1}{2}x+\sin x$, etc. These graphs seem to be of little value in themselves; and where there is a second part to the question: "Hence find the values of x for which the given expression is unity," the graphical method is often neither the most speedy nor the most accurate available.

The following course is tentatively suggested as one likely to prove of more interest and value.

1. The graphs of $\sin x$, $\cos x$, $\tan x$, $\cot x$, $\sec x$ and $\operatorname{cosec} x$ should be carefully studied first, the range of values of x extending from, say, $-360°$ to $+720°$, so that the periodicity, the continuity or discontinuity, and the maxima and minima of the functions are clearly seen. It would help greatly if the class could read in this connection Chapters XII. and XIII. of Whitehead's *An Introduction to Mathematics.*

The values of x (in radians), $\sin x$ and $\tan x$, when x is small, should also be studied in one large, carefully drawn graph.

2. It should then be shown how any two or more of these graphs can be combined by adding the corresponding ordinates or by adding multiples of these ordinates. This will prepare the way for the transformation of the expression $a\cos\theta+b\sin\theta$ into the form $r\sin(\theta+\alpha)$.

Numerical examples of this transformation ought to be worked in full so as to show that $3\sin x-4\cos x$, for example, may be expressed approximately either as $5\sin(x°-53°)$ or as $-5\sin(x°+127°)$, but not as $5\sin(x°+127°)$, though it is the last of these forms that the average boy will give unless he has been taught to check his results.

When the graph of $r\sin(\theta+\alpha)$ is known, some particular cases of the graph of $a\,.\,e^{bx}.\sin(cx+d)$ should be considered, as this function occurs in the theory of vibrations (compare Whitehead's *Introduction*, p. 215). A good example, $e^{-\frac{x}{2}}.\sin 3x$, was set in the Bristol Intermediate Examination for 1916. Even a more complicated example, such as $\frac{1}{4}e^{\frac{1}{10}x}.\sin(\frac{1}{3}x-\frac{1}{2})$, does not take very long to draw if $\alpha\equiv\frac{1}{3}x-\frac{1}{2}$ is regarded as the variable and the corresponding values of x and of the expression are calculated for $\alpha=0, \frac{\pi}{6}, \frac{\pi}{3}, \frac{\pi}{2}$, etc.

3. As regards curves in Higher Mathematics, the use of polar co-ordinates should first be taught. As Mr. Dobbs points out (p. 218 of his *School Course in Geometry*), paper specially suited to the use of these co-ordinates can be obtained.

Some of the following curves might then be plotted; and, if time permits, their most important properties should be indicated and some knowledge gleaned of their history.

(*a*) The Class involving simple relations between r and θ (in radians):

$r = a . \theta$ (the Spiral of Archimedes),

$r = \frac{a}{\theta}$ (the Hyperbolic Spiral),

$r = a^{\theta}$ (the Equiangular Spiral).

(*b*) The Class $r^n = a^n . \cos n\theta$:

Circle when $n = 1$,
Straight Line when $n = -1$,
Lemniscate when $n = 2$,
Rectangular Hyperbola when $n = -2$,
Cardioide when $n = \frac{1}{2}$,
Parabola when $n = -\frac{1}{2}$.

(*c*) The Class $r = a + b . \cos \theta$:

Limaçon when a and b are unequal,
Cardioide when $a = b$,
Trisectrix when $b = 2a$.

The choice of the particular values to be given to the various constants may be left to the individual members of the class, with perhaps the necessary warning that the graph when completed should " fill the page."

F. G. Hall.

Holloway County Secondary School.

GENERAL TEACHING COMMITTEE.

The Committee met on Saturday, 8th April, at 29 Gordon Square, W.C., and, among other business, passed the following resolutions:

Sequence in Geometry. That in the opinion of the General Teaching Committee of the Mathematical Association it is most undesirable that examining bodies should reduce the freedom of the teacher by imposing an obligatory sequence of propositions in Geometry.

Examination Questions. That a sub-committee be appointed to consider criticisms of mathematical questions set in public examinations.

Letters containing such criticisms should in future be sent to Mr. W. J. Dobbs, 58 Priory Road, South Hampstead, N.W. 6.

W. E. Paterson, *Hon. Secretary.*

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124. A yerd she hadde . . .
In which she hadde a cok, hight Chauntecleer,
In al the land of crowing nas his peer . . .
Wel sikerer was his crowing in his logge,
Than is a clokke, or an abbey orlogge.
By nature knew he ech ascencioun
Of equinoxial in thilke toun;
For whan degrees fiftene were ascended,
Thanne crew he, that it mighte nat ben amended.

(Per Sir G. Greenhill.) *The Nonne Prestes Tale*, 14853.

125. It was remarked by an eminent mathematician that while we give ourselves infinite trouble to pursue investigations relating to the motions and masses of bodies which move at immeasurable distances from our planet, we have never thought of determining the forces necessary to prevent the roofs of our houses from falling on our heads.—*Edin. Review*, vi. 386.

[Who was this ?]

MATHEMATICAL NOTES.

620. [X. 4. b.] *The Teaching of Parabolic Graphs.*

There is no doubt that the teaching of Algebraic graphs has much improved during the last decade. The ordinary teacher sees more clearly what he wants to do in such work and why he does it, so that now, one imagines, there is little of the aimless plotting of points which used to make this work so futile.

Quite apart from the mere ready-reckoner, utility aspect, graph work in the elementary stages of Algebra is essential to introduce the pupil to the idea of variation ; there is no other method which brings out so clearly the meaning of continuously changing quantities and the functional dependence of one changing quantity on another. Such ideas are, of course, more implicit than explicit at first, but the ground having been thus well prepared, good results may be expected later. At this stage, then, the actual plotting of the line or curve is very important and will occupy most of the pupil's attention.

This early stage quickly passed, the direct aim in graphical work shifts somewhat. The actual plotting has now no terrors ; the aim is to get the curve drawn as quickly as possible, and to proceed to the examination of it and its many and various implications. When dealing with parabolic functions, it is at this stage that the method of throwing the function into the form $y=a(x+b)^2+c$ (where a, b and c may be positive or negative) becomes particularly useful, and is, I think, far too little used in schools. Besides being a quicker than the usual method (as I shall show presently), it has this advantage, that the turning point of the graph is at once revealed. Anyone who has tried to teach parabolic graphs to children will at once appreciate this point.

The common-sense method is to start with $y=x^2$, the "head down" curve, and $y=-x^2$, the "head up" one. Let the pupils get to know the $2x^2$, $3x^2$ curves thoroughly, so that they memorise the points required for plotting each, and know what the curves look like with regard to each other. Now proceed to $y=x^2+1$ and $y=x^2+2$; $y=x^2-1$ and $y=x^2-2$, etc., which the pupil recognises as the old x^2 curve moved one or two units up and one or two units down respectively. The movements of the curve to the right and left must now be investigated, and $y=(x+2)^2$, for instance, is found to be the x^2 curve moved 2 units to the left, and $y=(x-4)^2$ the same curve moved 4 units to the right. After such work, a relatively complex function such as

$$y=-2(x-4)^2+6$$

is easily recognised as the "head up" parabola $y=-2x^2$, moved 4 units to the right and six units up. Pupils taught in this kind of way will learn more about parabolas in a couple of lessons than a month's work in point plotting could teach them. It is a simple matter to cut out a parabola in cardboard and, moving it about a squared blackboard, to demand from the pupils the corresponding equations.

The actual mode of procedure on the part of the pupil should, then, be as follows. When given a parabolic curve to plot, he should throw it into the form $y=a(x+b)^2+c$, and then draw the curve straight away. For example, given $y=2x^2-4x+6$, the corresponding form is $y=2(x-1)^2+4$, which is the curve $y=2x^2$ moved one unit to the right and four units up. Using the point $(+1, +4)$ as his new origin, he proceeds to draw the curve $y=2x^2$ at that point. The whole thing takes a few minutes, and the rest of the time may be more profitably used in the many results following from such a drawing. The number of questions arising is almost unlimited, and an intelligent class will quickly manufacture as many as are required. K. WHITEHEAD.

621. [K¹. 1. c.] *Morley's Theorem.*

ABC being any triangle with all its angles trisected: if the two trisectors of angle BAC intersect the adjacent trisectors of angles ABC, BCA in R and Q respectively, and if BR, CQ be produced to intersect in L, then

$$RL=QL.$$

For $\quad RL=BL-BR$

$$=BC\left[\frac{\sin\frac{2}{3}C}{\sin\frac{2}{3}(B+C)}-\frac{\sin C.\sin\frac{1}{3}A}{\sin A.\sin(60^\circ-\frac{1}{3}C)}\right]$$

$$=BC\left[\frac{\sin\frac{2}{3}C}{\sin\frac{2}{3}(B+C)}-\frac{\sin\frac{1}{3}C.\sin(60^\circ+\frac{1}{3}C)}{\sin(60^\circ-\frac{1}{3}A).\sin(60^\circ+\frac{1}{3}A)}\right]$$

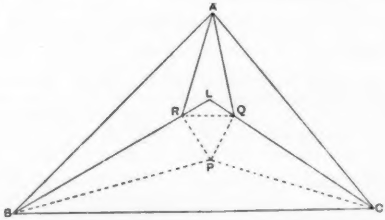
$$=\frac{BC.\sin\frac{1}{3}C}{\sin\frac{1}{3}(B+C)}\left[\frac{\cos\frac{1}{3}C}{\cos\frac{1}{3}(B+C)}-\frac{\sin(60^\circ+\frac{1}{3}C)}{\sin(60^\circ+\frac{1}{3}(B+C))}\right]$$

$$=\frac{BC.\sin\frac{1}{3}C.\sin\frac{1}{3}B}{\sin\frac{2}{3}(B+C).\sin(60^\circ+\frac{1}{3}(B+C))}$$

$=QL$, by symmetry.

This equality ($RL=QL$) affords immediate proof of Prof. John Morley's theorem that "the 3 points of intersection of the adjacent trisectors of the angles of any triangle form an Equilateral Triangle."

For, if—in accompanying figure—P be the third point of intersection, obviously, in the triangle BLC, PL will bisect the angle BLC, and the triangles



RLP, QLP will thus be equal in all respects, so that $PR=PQ$; and similarly it can be shown that $RQ=PR$ or PQ. C. H. CHEPMELL.

85 Wilbury Crescent, Hove, Sussex, 13th *February*, 1922.

622. [K¹. 1. c.] *Geometrical View of Morley's Theorem.*

Let $\alpha+\beta+\gamma=60$. Along the circumference of any arbitrarily drawn circle set off an arc ZY whose chord ZY subtends an acute angle of α degrees at the circumference.

This arc may be briefly designated by α.

Draw FE, WU, QP parallel chords to ZY, so that the intercepted arcs YE, EU, UP are designated by α, $\beta+\gamma-\alpha$, α respectively; and divide the arc $PQ(\beta+\gamma)$ in A so that $AP=\beta$, $AQ=\gamma$.

Then the angles WUY, UWZ standing on arcs of $\alpha+\beta+\gamma$ are each 60; and if UY, WZ are produced to meet in X, then XYZ and XUW are equilateral triangles.

Moreover, since arc $WQAU=2\alpha+\beta+\gamma=WFZYE$, therefore

$$WE=WU=UF.$$

Produce AF, UZ to meet in B; and AE, WY to meet in C. Then the angle $ABZ=AZU-FAZ=(\alpha+\beta)-\alpha=\beta$. Similarly $ACY=\gamma$.

Since BFA, BZU are secants through B to the circle,

$$AB : AZ = BU : UF = BU : UX, \text{ and angle } BAZ = BUX.$$

Hence the triangles BAZ, BUX are similar, and $UBX = ABZ = \beta$. Thus the triangles BFU, BXU are congruent and $BX = BF$.

Similarly $WCX = \gamma$ and $CX = CE$.

The angle $BXC = BAC + ABX + ACX = 3\alpha + 2\beta + 2\gamma = 120 + \alpha = QAP$.

Since $FBZ = \beta = PZA$ and $FZB = FAU = 2\alpha + \beta + \gamma = PAZ$, the triangles FZB, PAZ are similar and $BF : ZF = ZP : AP$.

In the same way the triangles EYC, QAY are similar and $EY : CE = QA : YQ$.

Hence $$BF(BX) : CE(CX) = QA : AP.$$

Thus the triangle XBC is similar to AQP, and the angle $XBC = AQP = \beta$;

$$XCB = APQ = \gamma.$$

We have thus a triangle ABC whose angles measure in degrees 3α, 3β, 3γ, and whose trisectors form by their intersections an equilateral triangle XYZ.

The same property must be true for any similar triangle by increasing or diminishing the scale of representation. R. F. Davis.

623. [I. 27.] *Note* 587. *Gazette*, p. 328, Oct. 1921.

A proof of the theorem stated by Mr. Richmond can be derived very simply from a principle enunciated by Dirichlet:

"*If $n+1$ particles are placed in n sockets, then there must be at least one socket which contains more than one particle.*"

Let θ denote any real number, and let

$$\theta_1, \theta_2, \ldots \theta_n$$

be the fractional parts of $$\theta, 2\theta, \ldots n\theta,$$

so that $\theta_r = r\theta - m_r$, with m_r integral and $0 \leqslant \theta_r < 1$. The interval $(0, 1)$ can be divided into the n equal sub-intervals

$$\left(0, \frac{1}{n}\right), \left(\frac{1}{n}, \frac{2}{n}\right), \ldots \left(\frac{n-1}{n}, 1\right),$$

it being understood that k/n belongs to the interval $(k/n, (k+1)/n)$ and $(k+1)/n$ to the next one. From the principle just enunciated it follows that at least one of these intervals must contain two (or more) of the numbers

$$\theta_0\ (=0), \theta_1, \theta_2, \ldots \theta_n.$$

If θ_r and θ_s belong to the $(k+1)$th interval,

$$\frac{k}{n} \leqslant \theta_r = r\theta - m_r < \frac{k+1}{n} \quad \text{and} \quad \frac{k}{n} \leqslant \theta_s = s\theta - m_s < \frac{k+1}{n}.$$

Hence, taking $r<s$ and subtracting these inequalities, the fractional part of $(s-r)\theta$ lies between $-1/n$ and $+1/n$. Now $s-r$ is one of the integers $1, 2, \ldots n$, so, taking $n=10$, the decimal part of $(s-r)\theta$ must begin either with 0 or 9. Again, taking $n = 10^t$, the decimal part of $(s-r)\theta$, with $0 < s - r \leqslant 10^t$, must begin with either t zeros or t nines. Decimal points can be inserted after any vertical row of figures in the illustration given by Mr. Richmond, so the theorem he stated is proved.

The University, Leeds. W. E. H. Berwick.

624. [S. 1. a.] *To find the depth of the centre of pressure of a triangle* (1) *with one vertex in the surface,* (2) *completely immersed.*

Let DAB be a triangle with one side DA in the surface of the liquid. Then the depth of the C.P. is half that of B. Let AC be a line through A meeting DB in C, and let the depths of B and C be q and r.

(1) Then the thrust on the portion ABC acts at a depth

$$\left[\frac{1}{2}qa \cdot g\rho\frac{q}{3} \cdot \frac{q}{2} - \frac{1}{2}ra \cdot g\rho\frac{r}{3} \cdot \frac{r}{2}\right] \Big/ \left[\frac{1}{2}qa \cdot g\rho\frac{q}{3} - \frac{1}{2}ra \cdot g\rho\frac{r}{3}\right]$$
$$= \tfrac{1}{2}(q^2 + qr + r^2)/(q + r) \text{ on reduction.}$$

(2) Draw any line CA' through C to meet AB at A', and let the depth of A' be p. The ratio of the areas of the triangles ABC and $A'BC$ is that of AB to $A'B$ or q to p.

The thrust on the portion $A'BC$ acts at a depth

$$[q(q^2 + qr + r^2) - p(p^2 + pr + r^2)]/2[q(q + r) - p(p + r)]$$
$$= \tfrac{1}{2}[\Sigma(p^2) + \Sigma(qr)]/\Sigma(p) \text{ on reduction.}$$

N. M. Gibbins.

625. [R. 2. b. γ.] *Centre of Gravity of a Pyramid.*

[Explain by general reasoning why the C. of G. of a pyramid must lie on a line from the vertex to the C. of G. of the base, and must lie at a constant fraction x, independent of the pattern of pyramid chosen, of the way from the C. of G. of the base to the vertex.]

Take three equal pyramids on square bases, with height equal to an edge of the base and vertex over a corner of the base. Put them together so as to form a cube (illustrating the volume of a pyramid). Call the edge of this cube 2.

Place the cube so that the vertices of its three component pyramids meet at a top corner of the cube.

Then the heights of the centres of gravity of the pyramids are $2x$, $1 + x$ and $1 + x$.

The mean of these must be the height of the C. of G. of the cube, which is unity. Hence $(2 + 4x)/3 = 1$; $\therefore x = \frac{1}{4}$. W. Hope-Jones.

626. [K[1]. 2. a.] *Note on Note* 605 (*Gazette*, xi. p. 20).

With reference to Mr. G. Srinivasan's elegant theorem it may be of interest to note that the centre of perspective O is the same point for all transversals parallel to the given one; so if the direction of the transversal is constant all the orthogonal circles touch at O. This follows at once from the facts that the pedal line of O for the triangle ABC is parallel to the transversal, and only one pedal line can be drawn in a given direction.

Draw the chord OS perpendicular to BC. Then $O\hat{S}A = O\hat{B}A = N\hat{L}B'$, so that LN is perpendicular to AS, and therefore to the pedal line of O.

1 Albert Road, Clifton, Bristol. E. P. Lewis.

627. [J. 2.] *A Problem in Probabilities.*

A—B, C—D, E—F, G—H, I—J, K—L represent 12 teams which are to play as stated on a particular day. Suppose P, a bookmaker, submits this list to Q, a backer, and challenges Q to predict three winning teams. Q, knowing his football well, picks out teams H, J, L, the three teams at the head of the League, as probable winners against G, I, K respectively, who as it happens are rather low down in the League, K in fact being at the bottom and its opponent L being near the top (say third). How can P calculate the fair odds that he should offer Q in a bet on the result? If the game between one or more of teams H, J, L should end in a draw, Q of course loses.

Secondly. In the above P does not know which three teams Q will choose; he lays odds against *any* three. Suppose he goes further and lays the further odds that whatever three teams are chosen by Q, none of these three will get more than a certain number of goals, say five. What would now be the correct odds to lay?

Thirdly. In first-class football the scoring is usually low. Suppose R plays against team S and that our friend P wishes to offer Q odds against Q's ability to predict not only the final score but the exact order in which the goals are

scored. Both P and Q are assumed to know the positions of R and S in the League and their previous scoring records. How can Q calculate the correct odds that P ought to offer him ? ARFON.

628. *Note on* 617 [V. 1. a. μ.] *Vector Proof that the Mid-points P, Q, R of the Diagonals AC, BD, EF of a Complete Quadrilateral are collinear.*

(In the figure $ABCD$ was a convex quadrilateral; AB met DC in E, BC met AD in F.) The resultant of four forces AB, AD, CB, CD is shown to pass through P, Q, and R, which are therefore collinear.

The method also proves that the centre of any conic which touches the sides of the quadrilateral lies on PQR. For if a conic with centre O touch AB, BC, CD, DA in X, Y, Z, W respectively, the resultant of forces AX, AW acts from A to the middle point of XW. But XW, the chord of contact of tangents from A, is known to be bisected by the diameter AO. Hence the resultants of the pairs of forces AX and AW; XB and YB; CY and CZ; ZD and WD all pass through O, *i.e.* the resultant of AB, CB, CD, AD passes through O.

Where the conic flattens into the line EF, X and Z lie at E, Y and W at F, and O lies at R. This gives a proof that R lies on the line, which differs a little from that given by Mr. W. J. Dobbs.

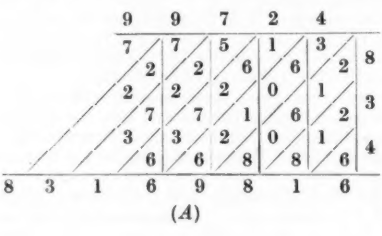
It would be interesting to discover where this simple proof originated. I learned it myself in 1882 from Dr. Routh, who had probably given it to his pupils for many previous years. H. W. RICHMOND.

629. [V. 1. a. μ.] *Question proposed for Solution.*

Show how to resolve a given force into three co-planar forces of given magnitudes acting respectively through three given points. W. J. DOBBS.

630. [A. 1.] *Note on Note* 598.

Hindoo Long Multiplication. This is the name under which a process essentially equivalent to that given by Mr. Meadows on p. 14 of the present volume of the *Gazette* was described, with comments on its merits and a reference to Cajori's *History*, by Prof. G. H. Bryan in the *School World* for August, 1907 (vol. 9, p. 284). The work was there set out in the form (A) below, the addition being diagonal. For some time before 1907 I had been using the form (B), which is more compact than Mr. Meadows' and better adapted than (A) to the very common cases in which a number of products have to be added. In these cases, of which the simplest example is the picking of a specified coefficient from the product of two polynomials, the additions in the individual products are not performed, but personally I find it worth while to make separate additions of the positive terms and the negative, as indicated in (C), which shows the calculation of the coefficient of Y^2y in the numerator of the expression equivalent to N/Z^2 in Note below as it appears in my rough notes; a product by 5 or by 2 is written down in one line, as is any other term, such as 12421×4, which happens to be simple enough.



(A)

99724
834
22662
77513
77162
22201
66886
33201
83169816

(B)

12140
2428
3794
9485
3471
222
2666
133
2661
+ 965260
− 404928
−1370188
778
114
222
996
445
664
556
24842
49684

(C)

E. H. NEVILLE.

631. [K¹. 11. e.] *Note on a Porism of Lord Brougham.*

Recently I have come across Lord Brougham's *Tracts Mathematical and Physical*, wherein is reprinted a paper entitled "General Theorems, chiefly Porisms, in the Higher Geometry," from the *Philosophical Transactions* of 1798. Among others he gives the following statement of a Porism:

"Two points in a circle being given, but not in one diameter, another circle may be described, such, that if from any point of it to the given points straight lines be drawn, and a line touching the given circle, the tangent shall be a mean proportional between the lines so inflected. Or, more generally, the square of the tangent shall have a given ratio to the rectangle under the inflected lines."

Treated as substantive propositions, Brougham's statements are capable of easy proof by elementary geometry. Considered from the point of view of a Porism they are incomplete. If A and B be two fixed points, and OT the tangent from a moving point to a fixed circle; then the locus, defined by the condition $OA \cdot OB = k \cdot OT^2$, breaks down into two circles in two different ways; firstly when A and B are on the circumference of the fixed circle, as in Brougham's statement, and secondly when A and B are inverse points with respect to the said circle.

The second of these cases is capable of generalisation. Suppose that three circles are given, and that we desire to know in what way the locus of the point O, which moves in such a manner that the lengths of the tangents from it to the three circles are connected by the relation $OT_2 \cdot OT_3 = k \cdot OT_1^2$, may break down into a pair of circles, we find a case controlled by two conditions. Firstly, the three centres must be collinear. Secondly, if we denote the positions of the centres by A, B, C, and the radii of the corresponding circles by a, b, c, the other condition may, according to the conventions of modern geometry, be written in the form

$$a^2 \cdot BC + b^2 \cdot CA + c^2 \cdot AB + BC \cdot CA \cdot AB = 0.$$

This is a truly Poristic case, as the conditions do not involve k. It is to be remarked that, although the second condition is symmetrical, we cannot associate our fixed circles indiscriminately in the formation of the condition $OT_2 \cdot OT_3 = k \cdot OT_1^2$.

When the fixed circles satisfy these conditions, the three polars of any point with respect to them are concurrent.

The only alternative to this, supposing the conditions satisfied irrespective of the value of k, is that two of the circles should be reduced to points. There would seem to be other cases, defined by relations among the circles, for which the locus breaks down into a pair of circles, but only for particular values of k dependent on the special conditions satisfied by the original circles.

J. Brill.

632. [K¹. 20. e.] *The Ambiguous Case.*

(i) The case of congruence of triangles which agree as to two sides and a right angle not contained is at present regarded by examiners as essentially distinct from the other cases.

If two triangles agree as to three sides, when the equalities of the three sides have been stated and justified nothing remains for the examinee to do before stating that the triangles are congruent; but if they agree as to two sides and a right angle not contained, he must, if he is to avoid losing half his marks, either have commenced by saying "in the right-angled triangles" or finish by adding "these are two sides and a right angle" before he can conclude that the triangles are congruent.

[It is true that some examiners draw a subtle distinction between the statements "$A = B$, right angles" and "A and B are right angles," by which the examinee may, probably by pure luck, be saved from loss.]

Why this severity? There is no ambiguity about the congruence of two triangles which agree in this way. Why should it be necessary to say "these are two sides and a right angle" when in another case it is not necessary to say "these are two angles and a side suitably placed"? Might not the examinee who has stated and justified equalities which ensure congruence be permitted to infer congruence without adding (by implication) that he is aware that if the conditions had been different there might have been ambiguity?

(ii) Is it not time that the ambiguous case in congruence of triangles should be more freely used? If two triangles agree as to two sides and an angle not contained, then another pair of angles are either equal or supplementary, so that if they are known not to be supplementary they must be equal and the triangles congruent.

It is not hard for beginners to learn which the angles are. One pair of equal sides have the given angles opposite them. It is the angles opposite the other given sides which are either equal or supplementary.

Probably the best rider which uses this case of congruence is "to prove that if the bisector of the vertical angle of a triangle also bisects the base, then the triangle is isosceles."

This rider is often done by producing the bisector an equal distance, but it is quite unnecessary to make this construction. From the data we infer that the base angles of the triangle are equal or supplementary, and as the latter is impossible unless the vertex is at infinity, the former is true and the triangle is isosceles.

In a recent School Certificate paper a line was drawn through the internal centre of similitude O of two circles (the technical term not being used), and met the circles for the first time at P and Q, a figure being given. It had to be proved that $PO:OQ=$ratio of radii. This is the ambiguous case in similarity, but if the circles were equal would become that in congruence.

As a last example: ABC is equilateral, $BDEC$ a square on BC externally, AD cuts BE at O. Prove angles OAC, OEC equal, and hence, using the ambiguous case, prove triangles AOC, EOC congruent. C. O. Tuckey.

633. [L^1. 14. a.] *C. Smith, "Conics (Coord. Geom.)," chap.* xiii. *no.* 16.

A conic is inscribed in a triangle so that its centre is at the circumcentre. To prove that its axes are $R+d$ and $R-d$, where d is the distance between the circumcentre and the orthocentre of the triangle.

(Proof by pure geometry.)

Let C and O be respectively the circumcentre and orthocentre of the triangle STU, and P, Q, R the points of contact of the inscribed conic. Let SO cut TU in K and the circumcircle in L; let OH perpendicular to OU cut TU in H; and let E be foot of perpendicular CE on TU. Then $\triangle CPT=\triangle CTR$, and similarly for the other vertices.

$$\therefore\ \triangle CPT+\triangle CSU=\tfrac{1}{2}\triangle STU.$$

But quadrilateral $SCKU=\frac{1}{2}\triangle STU$, for $\triangle SCK=\triangle SEK$.

$$\therefore\ \triangle CPT=\triangle CKU;\quad \therefore\ TP=KU.$$

If RT, $R'T'$ (T' on TU) are parallel tangents to the conic, the projections perpendicular to ST of TT' and HU (CG perpendicular to ST) are respectively $2CG$ and OU, which are equal.

$$\therefore\ TT'=HU;\quad \therefore\ PT'=HK;$$

$\therefore$ CD being the semidiameter conjugate to CP,

$$CD^2=PT\,.\,PT'=HK\,.\,KU=OK^2;\quad \therefore\ CD=OK.$$

Now it is a known theorem that if, on the normal at P, points J and J' be taken so that $PJ=PJ'=CD$, $CJ=a+b$ and $CJ'=a-b$.

But since $TP=KU$ and $CD=OK=KL$, J is evidently on the circumcircle and CJ' is symmetrically equal to CO.

Hence $a+b=R$, $a-b=d$; whence the result. E. H. Smart.

REVIEWS.

Three Lectures on Fermat's Last Theorem. By L. J. Mordell. Pp. viii + 31. Price 4s. net. 1921. (Cambridge University Press.)

With one exception the arithmetical results stated by Fermat, such as the fact that every prime number of the type $4n+1$ is expressible as a sum of two squares (*e.g.* $113 = 7^2 + 8^2$), were proved within a hundred years of his death in 1665. The solitary exception, which has achieved fame as the *Last Theorem*, states that, when n exceeds 2, there exists no triad of positive integers x, y, z, such that $x^n + y^n = z^n$.

The truth of the last theorem has been demonstrated for certain smaller values of n (omitting only 37, 59, 67 as far as 100), but none of the proofs has led to a complete generalisation as yet. Additional prominence has been given to this theorem during the last few years through the establishment of a prize of 100,000 marks for the first complete proof that Fermat's statement is either true or untrue.

Mr. Mordell, in the course of three public lectures delivered at Birkbeck College in 1920, gave an account of the present state of knowledge concerning the Last Theorem. These lectures (with some amplifications), now printed for the benefit of a wider public, give a very lucid account of what has been done, and trace the fallacies in some of the better known and obvious attempts to prove the theorem.

In view of the considerable erroneous literature on the subject, it is to be hoped that any one proposing to make a serious study of the Last Theorem will make himself acquainted with Mr. Mordell's pamphlet and equally with the last chapter in Vol. 2 of Prof. L. E. Dickson's *History of the Theory of Numbers*. For the sake of those desiring to complete the proof, it is only fair to say that it has been unsuccessfully attempted by the greatest of mathematicians during the last two centuries, including Euler, Legendre, Gauss, Cauchy and Kummer, and has several times been made the prize question of learned societies.
W. E. H. B.

Real Mathematics. By E. G. Beck. Pp. viii + 306. 15s. 1922. (Henry Froude, Hodder and Stoughton, Oxford Technical Publications.)

This does not profess to be a text-book, and, "so far as the Author is aware, no such treatment of mathematics has been published previously." Its objects, as stated in the preface, are "to offer assistance to practical engineers and engineering students in the acquisition of a real, serviceable, and sound mathematical equipment," "to augment the standard text-books and orthodox methods of study," and to provide "a standpoint from which the subject may be studied throughout in the light of simple and straightforward human reality." The portions of the subject selected for treatment are: arithmetic (including logs.), algebra (mainly factors, equations and graphs), trigonometry, and the fundamental ideas of the calculus, concluding with a chapter on "Units, Fundamental and Derived."

The preface and introduction—and indeed the whole book—are full of moral precepts. The introduction is almost psycho-analytic, and we are presented with a harrowing picture of the engineer suffering from a buried complex, due to his lack of power of self-expression in mathematics. This is diagnosed as the effect of the "old trouble, the glorification of technique into an end, instead of its utilisation as means to an end." There is a deal of truth in this, as anyone who has had to teach mathematics to engineering students will agree. Most mathematical text-books are written by mathematicians, who are more concerned with the principles of the subject than with its applications. Engineers are perforce practical, and that which is not practical does not appeal to them; moreover, they have little time to "waste" on non-essentials. Convince them of its usefulness, and let them see how and to what it can be applied, and they will be found to be as keen on mathematics as anyone. There is still ample scope for someone who understands and appreciates the mental outlook of both mathematicians and engineers, to present mathematics in such a manner that the acquisition

of sound ideas and facility in manipulation may be secured through engineering applications. But to return to our author.

"Real education cannot be grafted on to a man from without by pumping into him formulated statements of fact—no matter how true, precise, and comprehensive these statements may be."

"To represent a thing by a symbol is not equivalent to transforming the symbol into the thing it represents—and what a world of trouble would be prevented, in all departments of life, if human beings appreciated this simple fact."

"We cannot add to one group without subtracting from another, and usually the subtraction must be effected before the addition becomes possible. This fact is of enormous importance in the affairs of everyday life."

"The ability to solve a differential equation is, of itself, not worth five seconds of effort to acquire."

"Let it be remembered that graphs are merely the rough 'sighting shots' of the real engineer."

"In surveying, the best way to deal with the ambiguous case is to ensure that it shall not arise."

"Mathematics is to the engineer what accountancy is to the honest trader."

"Mathematics is concerned with one root principle only—*the counting of real things in groups*."

As far as the last two statements are immediately applicable, the author has some good things to say. The rule of signs is carefully treated, on the basis of assets and liabilities. The correlation of sign with direction is, however, hardly mentioned, though this would seem to be of prime importance in engineering applications. Possibly he is afraid that to call a force or a distance negative will deprive it of its "reality." Square root and algebraic factors are done diagrammatically, and the suggestion is made that they can quite well be done "on a Halma board." Another useful idea, for teaching logarithms, which is, so far as we are aware, new, is to regard addition and subtraction of logarithms as a petty cash account. The mantissae represent the petty cash itself, while the characteristics are regarded as advances or payments from or to the banking account.

Trouble arises with involution and evolution. We are carefully told that "x" NEVER stands for a length (or, any other quantity—implied) but always for *a number of real things*. The author does not seem clear whether the "things" are implied in the "x" or not, though he is dogmatic "that numbers may not be dealt with apart from assemblages,"—"the action of counting can no more be divorced from the things counted than can the action of eating from the things eaten," and at the same time is equally clear that 5 apples $\times$ 8 buns is nonsense. As a result of this, we are asked, because a man who has to visit two farms, at each of which there are two pig-sties, each containing two pigs, each requiring a meal of two potatoes, must set out laden with 2^4 potatoes, to notice that there is nothing "four-dimensional" about it. The 2's are merely factors of repetition. Why this 2^4 is 2^4 potatoes, and not sties or farms or pigs, is not stated. It is too obvious! On such argument is based the statement that "the nth root of a number is of the same nature as the number, and so, also, is the nth power of that number. The square root of 100, if the 100 things be *men*, is 10 *men*; the square root of 9 *pencils* is 3 *pencils*; the square root of an *area* is an *area*. . . . For the square root of a negative quantity, the solution is even less difficult, . . . obviously, $\sqrt{-25}=-5$". On the same lines, the solution of the equation $5x^2+11=-234$ is shown to be $x=\{+(-7)\}$ or $x=\{-(+7)\}$, or, more concisely, $x=\{\pm(\mp 7)\}$.

Is it legitimate to say, following this argument, that the time taken by a body let go to rise 64 ft. is $\pm(\mp 2)$ secs.? At the same time the equation $x^2+12x+40=0$ has no solution (though by putting $x+6=y$ it can be stated in the form $y^2-36=40$, so surely one might say, "concisely," that $x=\pm(\mp 2)$).

On the question of derived units, the author also holds some startlingly heterodox views. For instance: "If we find that liquid flows from a pipe at the rate of 3 gallons a minute, the discharge is simply 3 *gallons of liquid*. . . . Failure to appreciate this fact has led to many errors, notably that the Moment of Inertia (of a beam section) is "four-dimensional," and that the Section

Modulus is "three-dimensional." In support of these statements we are referred to the author's *Structural Steelwork* for a demonstration that "the Moment of Inertia and the Section Modulus are ordinary lengths, measurable in ordinary inches, and that the 'ratio of stress to strain' has no part in the significance of the Modulus of Elasticity."

"It is important to remember that, after all, a velocity is only a distance."

". . . acceleration is really a simple thing, involving nothing more than measurements with a tape—and a stop-watch—and certainly calling for no such feats as the division of feet by square seconds."

Of course, he has the usual engineer's quarrel with the poundal, and gibes at the physicist by remarking that "even when the unit of mass is deduced (*sic*!), it is really a weight." It is with no surprise that we then learn that "the earthward acceleration of a body is not affected by its weight." Comment is unnecessary, and we conclude with another extract from the preface, "much more is available if required." W. G. Bickley.

A Short Course in Commercial Arithmetic and Accounts. By A. Risdon Palmer. Pp. 171+xv. 2s. 6d. 1922. (Bell & Sons.)

When we find on the first two pages that the Complementary, or Shop, Method of Subtraction, and the use of Rough Checks, are insisted on, we are prepared for all the good things that follow in this excellent little volume; all the more when we find, on p. 5, that the method of multiplication in which the digit of the multiplier of highest place value is used first is given to the total exclusion of the ordinary method. But why is it then necessary to place the multiplier beneath the multiplicand according to some definite rule? This is quite unnecessary when a preliminary rough answer has been obtained.

There is but one adverse comment; and that is, that in our opinion it would have been advisable either to omit the section on Foreign Money, or to give the up-to-date rates of exchange, the price of silver and present-day silver coin alloy. We cordially draw attention to the merits of the text-book before us, for which we hope there will be a large demand, and that hope is in the interests of the rising generation of commercial students. J. M. Child.

Practical Mathematics. Part I. By A. Dakin. Pp. 362+12 (tables), +xxiv (answers). 5s. 1922. (Bell & Sons.)

This is one of the finest introductions to practical mathematics that have ever come under our notice; the text is well written and clearly expressed, the diagrams are excellent, the examples are well graded and numerous enough for the most exacting requirements. Some people, of whom the reviewer is certainly not one, will perhaps grumble at the use of decimals to the almost total exclusion of vulgar fractions. Some teachers may prefer that the methods of approximate multiplication, division, etc., should come in much earlier, and not be relegated to a final chapter. It is in the power of an author, and should be his aim, to set the fashion and not to follow it. Mr, Dakin heads his chapter with the rubric: "For those teachers who desire their students to have a knowledge of the methods." Surely no teacher, who considers that all measurements, and therefore all practical mathematics, are approximate, can fail to see the absolute importance of approximate methods. Again, a table of logarithms should have been given; it is so easy and appropriate for beginners to be taught the *use* of logarithms without allusion to their connection with indices. The data, that the logarithms of 1 and 10 are 0 and 1 respectively, and that the product of two numbers has a logarithm equal to the sum of the logarithms of its factors, are all that is required to obtain all the laws of logarithms—and this method, moreover, is more historically correct. Another small point, and yet an omission that is to be (one might almost say) deplored, is that no mention seems to be made of "standard form," to which a student cannot be introduced too early after he has gained an insight into decimals.

These are, however, merely errors of omission; we have found no errors of commission. One final remark we must make, and that is that we have never seen such a fine set of interesting and stimulating exercises, and most of them are original. We confidently recommend this book to every teacher of elementary practical mathematics. J. M. Child.

Practical Mathematics for Central and Continuation Schools. By G. Simmonds. Pp. 212, with Answers. Price 4s. 6d. 1922. (Methuen.)

The author of this little volume is convinced that accurate and ready measurement forms the real basis of Mathematics. Even if we wholly agree with him in this, such sweeping generalisations as occur on pp. 18, 19 cannot be accepted as good teaching. A similar remark applies to the attempt to justify the Rule of Signs, although the serial method combined with observation (which is used) has much more force than the usual argument of our school algebras. We should like to see the modern forms of setting out G.C.M. and L.C.M. used, and the old-fashioned word "trapezoid" replaced by "trapezium." Some of the examples require attention; several are of the type "If it takes a clock three seconds to strike three o'clock, how long will it take to strike six o'clock?" Judging by the answers that are given, the author would say that it took six seconds to strike six o'clock.

Apart from such faults and slips, the book has much to recommend it among the schools for which it is designed; but the name, "Practical Mathematics," which seems to denote that it is suitable as a preliminary course to the usual treatises used in technical colleges, is hardly justified.

J. M. Child.

Cours complet de mathématiques spéciales. Tome II. Géométrie. By J. Haag. Pp. 658. 1921. (Paris: Gauthier-Villars.)

The strong point of this *cours* is the unusually wide range both of subjects and of methods. Two and three dimensions are mostly treated together, and where the arguments are identical, they are given for space only. Elements of first and second degree by no means monopolise the book; and pure, analytical and differential methods are used wherever most suitable, and their relations brought out. There are interesting sections on such topics as transformations, congruences, and a few special plane and twisted curves and surfaces, but their usefulness is lessened by certain obsessions of the author: orientation of lines and surfaces (he distinguishes between a circle and a *cycle* or directed circle), parametric representations, the circular points at infinity. The balance is strongly inclined to metrical rather than projective properties.

The proof-sheets must have been revised with special care, to judge both from the absence of misprints and from the number of remarks added as footnotes that should have been in the text. The figures are poor and too small. There are no references, and the reader's knowledge is assumed to consist exactly of the contents of the first volume.

The style is anything but French; in the earlier chapters especially, there is a laboured completeness that is excessively tiresome, though sometimes relieved by an abrupt change of topic, as from the ordinary Cartesian equation of a circle to lines of zero length. The repeated exhortations to learn by heart are also tiresome, and the reference to the "fâcheuse habitude" of pupils will appeal to bad teachers rather than good. H. P. H.

Modern Electrical Theory, *Supplementary Chapters*: Chapter XV., **Series Spectra.** By Norman R. Campbell, Sc.D. Pp. viii + 109. 1921. 10s. 6d. net. (Cambridge University Press.)

Modern research is proceeding so rapidly that it is a matter of the greatest difficulty for the student not actually engaged in it himself to keep in any way in touch with the march of progress, even in its most fundamental directions. The non-specialist is largely ignorant of what is being done, or even in what direction work is being carried on, until some important new discovery is announced, and then he is apt to find very real difficulty in realising what it is all about, how it has occurred, and in just what its importance consists.

It was to meet these needs in one branch of discovery that in 1907 Dr. Campbell produced his *Modern Electrical Theory.* The second edition in 1913 was virtually a new book, the original having been almost entirely rewritten, and now Dr. Campbell has adopted the happy and original device of reissuing it piecemeal as a series of monographs dealing with the most important advances in physics since the last edition, the series to be continued whenever the need arises, the monographs forming, according to circumstances, either

additional chapters or old chapters rewritten. The first of this new series forms an additional chapter dealing with Series Spectra, and the author states quite clearly in the preface that the work "is still intended, not for experts, but for students who, having taken the usual examination courses, wish to get into touch with research. It aims at giving them such knowledge of the chief regions in which research is active that they may proceed at once to the original memoirs; much more attention is therefore given to the clear exposition of the main ideas which inspire the research than to detailed statements of the results that have been obtained from it."

The writer has long been of opinion that this is work that most emphatically needs doing. The vast majority of students after graduating are not in a position to make any direct contribution to the advancement of knowledge; but they should be the chief means to its dissemination, and the universities and other centres of learning, and a real Board of Education should take steps to maintain their intellectual life and to combat the rapid decline that usually sets in. Probably the best means to this end are monographs such as the one now before us, and those familiar with Dr. Campbell's writings will not need to be assured of the skill and charm with which he has again accomplished his purpose; indeed the present writer confesses to a feeling of happy excitement aroused on more than one occasion by examples of his masterly exposition. It is, in fact, remarkable how the author has succeeded in making intelligible to one not brought up on such things the ideas and implications of states of an atom, quantum numbers, conditionally periodic orbits, relativity and quantum theory, and the like.

It is interesting, and perhaps significant, to remark that by far the most difficult section of the book to the non-expert reader is that dealing with just those branches in which the Bohr-Sommerfeld theory—the main concern of the book—has hitherto proved least successful, *i.e.* the explanation of the intensity of spectral lines, with its dependence on probability and the principle of correspondence, where the discussion is more purely mathematical with a less intimate physical basis. B. M. N.

Introduction to the Mathematical Theory of the Conduction of Heat in Solids. 2nd ed. By H. S. CARSLAW. Pp. xii + 268. 30s. net. 1921. (Macmillan.)

This book is the second volume of the new edition of the author's book on *Fourier's Series and Integrals and the Mathematical Theory of the Conduction of Heat.* (For a review of the first volume of the new edition, see the *Mathematical Gazette* for October, 1921.)

Extensive alterations and additions have been made. Chapters XI. and XII. are quite new. The former deals with contour integrals and the latter with integral equations. Contour integrals are perhaps the most powerful weapons that can be found for attacking problems in the conduction of heat. They are quite as useful in practice as the operators used by Heaviside, and in fact are simpler to work with. Moreover, the results established by them are beyond doubt, whereas Heaviside's methods contain many steps that are difficult to justify, except by reference to contour integration. This chapter is an extremely valuable one.

Those who have no previous knowledge of integral equations will find Chapter XII. an interesting introduction to the subject. From the purely physical point of view, however, integral equations do not seem to have accomplished very much at present. In the discussion of the linear flow of heat by means of them, the emphasis is on the necessity for proving that a certain function *can* be expanded in the required series. Fourier assumed this as obvious, and the student of experimental physics will probably do the same. Of course to the pure mathematician the point is an extremely important one, and by settling it in a satisfactory manner integral equations have been of great service. But the subject is by no means easy, especially in the extensions to two or more dimensions. Prof. Carslaw gives a brief indication of these extensions and points out the difficulties that arise.

An interesting footnote on p. 60 brings the account of the Age of the Earth controversy up to date, even mentioning the British Association discussion of September last. Great care has been taken to revise all the references.

Two small errors in the first edition have been left uncorrected : some plus and minus signs on p. 92 have been interchanged, and Clerk Maxwell is given a hyphen. Apart from these trifles the book is an excellent one, written by one who has contributed much himself to this branch of mathematics. (It may be called mathematics rather than physics, for students of experimental physics now ignore it almost entirely. This is no doubt chiefly due to their great interest in electrons and atomic structure, but possibly many are frightened by the mathematical difficulties involved in the modern treatment of the conduction of heat.)

The publishers and printers deserve praise for the clear type and good paper, but the price is a high one. The first edition cost 14s., while the two volumes of the second edition together cost 60s. This seems too great an increase, and it may prevent the book having the wide circulation that its merits deserve.

H. T. H. Piaggio.

Obituary.

G. B. Mathews, F.R.S.

THE PILLORY.

A ferryman rows a boat at 5 miles per hour in a N.E. direction through the water of a river flowing S. at 2 miles per hour. What is the velocity of the boat relative to the bank of the river ?

A man is walking along the bank at 3 miles per hour due S. By how much will the distance between the boat and the man increase in one minute ?—University of London, *General Schools Examination*, Midsummer, 1921.

THE LIBRARY.

The Library has now been removed to 29 Gordon Square, London, W.C. 1 and Mr. W. E. Paterson has taken over the duties of Honorary Librarian.

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THE MATHEMATICAL ASSOCIATION.

(*An Association of Teachers and Students of Elementary Mathematics.*)

"I hold every man a debtor to his profession, from the which as men of course do seek to receive countenance and profit, so ought they of duty to endeavour themselves by way of amends to be a help and an ornament thereunto."—BACON.

THE MATHEMATICAL ASSOCIATION, which was founded in 1871, as the *Association for the Improvement of Geometrical Teaching*, aims not only at the promotion of its original object, but at bringing within its purview all branches of elementary mathematics.

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